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Artificial Intelligence 171 (2007) 1094–1103

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**Artificial  
Intelligence**

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[www.elsevier.com/locate/artint](http://www.elsevier.com/locate/artint)**Book review****Symposium on Margaret Boden, *Mind as Machine: A History of Cognitive Science*, Oxford, 2006, two volumes.****Noam Chomsky***MIT, USA*

Available online 11 October 2007

When I was asked to join a symposium on Margaret Boden's history of cognitive science, I demurred, explaining that I felt it was inappropriate, given the role assigned to me in her saga. After several requests, I agreed, but with the same reservations. I'll therefore keep to my assigned role as the demon who almost destroyed the field, though fortunately it was saved, just in time, by a few courageous souls who escaped my iron grip and were able to "trounce" my own failed efforts, and even to "eclipse" all of linguistics, rescuing cognitive science from disaster.

It's an exciting story, no doubt, and I hate to be a spoilsport. But there are a few problems. One is that virtually every reference to me or to (unidentified) co-workers around the world, and to the areas in which we work, is fanciful, sometimes even bringing to mind Pauli's famous observation "not even wrong." I'll review what seems to be a fair sample.

I should point out, in clarification, that I'll barely touch on the topics that have been of primary interest to me in linguistics, philosophy, and cognitive science (as I have understood the field since its revival in the 1950s). The reason is that Boden alludes to these topics only tangentially, if at all. That is no criticism. She is entitled to her own choice of topics, and is under no obligation to discuss mine.

Boden opens her account of the role she assigns to me by formulating "The tenfold Chomsky myth," which it will be her task to refute. She mentions no one who ever articulated these myths, or hinted at them. Or who might even believe any of them. For good reasons.

After this auspicious start, Boden launches her campaign. Its target is not merely a construction she calls "Chomsky," "the high priest of a new orthodoxy," but more generally a band of "Chomskyans," unidentified and not cited, but extremely powerful as well as malign. They dominate the field of linguistics, though a few corners have escaped, one housing her and her colleagues, the source of much of what she relates, as she reports. Furthermore, according to the sources she has unearthed, "Chomskyan cliques" not only "came to dominate various conferences and journals," but also police the field with rigor and "ruthlessly" ban dissidents. To this day heretics "are 'locked out' of the many departments of linguistics still dominated by the Chomskyan paradigm," though they still struggle elsewhere.

Boden's total evidence for these charges reduces to a single case: a leading contributor to generative grammar who "was systematically reviled and excluded by his so-called ex-colleagues," referring presumably at least to my department at MIT. The reviled and excluded linguist in question was "locked out" by being welcomed in our department as a full professor through the years of his most outspoken heresy and has been constantly cited with great respect to the present day, as even a casual look at the "Chomskyan" literature reveals (and, irrelevantly, keeping friendly personal contacts). In the 1980s, a prominent advocate of Boden's favorite theory, her colleague Gerald Gazdar's GPSG, was "locked out" by being repeatedly invited to visit and teach in our department and offered a permanent position as full professor, has had regular work contacts with "Chomskyans" (sometimes even the demon himself), even co-published with some of them, and is constantly cited by them. And happens to be a personal friend. And on, and on.

Boden's sources also deplore the "hostile takeover of the whole damned system" by "intellectual Brahmanists" who insist that work must be "American"—which will amuse linguists abroad, including England, who are aware

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*E-mail address:* [chomsky@mit.edu](mailto:chomsky@mit.edu).

of the major centers in Europe and the annual meetings of GLOW (Generative Linguistics in the Old World) and other international organizations, as well as in Japan, Korea, Israel and elsewhere. “The Master” is able to contain the orthodoxy within home shores by relying on the “powerful magic” of the “Apollo program and the billion dollar economy [sic].” The ultra-nationalistic “Chomskyans” dismiss “solid work done in the ‘wrong’ places.” They inspire such terror that critics have “preferred to remain anonymous,” fearing that their careers would be ruined if they dared to speak out. Meanwhile, the acolytes “pervert” the discipline in a “sick manner” by pursuing “their strange little version of navel-gazing, free from any contamination from the real world” of linguistic fact.

So it continues, invariably without the taint of evidence.

Though the flood of invective is impressive, Boden says virtually nothing about the work that inspires her rage and ridicule, either mine or that of the participants in the vast “Chomskyan” conspiracy—vilified, but unnamed, an ugly insult to hundreds of serious and productive researchers.

Plainly, it is impossible to respond to such rhetorical flights. Nevertheless, some comments about her general method.

To begin with, Boden does not seem to comprehend the terms she uses. Thus she refers repeatedly to my “postulation of universal grammar” (UG) and writes “What universal grammar will turn out to be—if it exists at all—is still unclear.” UG is the term that has been used for many decades to refer to the theory of the genetic component of the human language faculty, whatever it will turn out to be: very much an open question, of course, as in far simpler cases that are far easier to investigate. Insect communication, to mention one. To question the existence of UG, as she does, is to take one of two positions: (1) there is no genetic component; (2) there is one, but there is no theory of it. We can presumably dismiss (2), so Boden is left with (1). She is therefore questioning the existence of a genetic factor that played a role in my granddaughter’s having reflexively identified some part of the data to which she was exposed as language-related, and then proceeding to acquire knowledge of a language, while her pet kitten (chimpanzee, songbird, etc.), with exactly the same experience, can never even take the first step, let alone the following ones. It is either a miracle, or there is a genetic factor involved. Boden’s suggestion—presumably unwitting—is that it may be a miracle.

If UG is viewed as a mapping from external data to internal state attained (abstracting from more general principles of growth and development), we can think of it as a “language acquisition device” (LAD). To question the existence of LAD is the same as to question the existence of UG—that is, to leave my granddaughter’s feat a miracle. Boden questions the existence of LAD; more accurately, thinks she does. She cites “non-Chomskyans” who believe that the capacity to acquire language “may be some combination of mechanisms evolved for more general purposes.” If this shot in the dark happens to hit the mark, then LAD is that combination, and Boden again agrees that LAD/UG exists, while believing that she questions its existence.

In the same connection, Boden writes that Quine issued a “challenge” that “Chomsky couldn’t meet” even 20 years later [26]—or, she could have added, for the indefinite future: to present UG in full. The comparable “challenge” has not been met anywhere in the field, or for that matter in the sciences generally, and if posed would be dismissed with disbelief. If she means that “Chomskyans” have made no progress in developing UG, she does not even pretend to justify that charge, so we can drop the matter.

Boden also writes that “Chomsky himself changed his mind more than once about just what the ‘universal’ base of language is”—that is, about the nature of UG. Even more scandalous, he changed my mind so radically by the 1990s that he proposed “shocking” conclusions about grammaticality, which she quotes—namely, those that are discussed in a full chapter of Chomsky ([5], *LSLT*), and repeatedly later. Since Boden says she could not comprehend *LSLT*, there is a discussion in a technical paper on language use, a topic that she regards as within her domain ([23], which she cites). In brief, the “shocking” conclusion is a brief reference to what was developed in detail 40 years earlier, and repeatedly discussed in books and papers of the 1950s and 1960s, to some of which she refers.

Chomsky’s “last gasp” (apparently, before taking off into outer space) was in the early 1980s, the Principles and Parameters (P&P) approach. She fails to mention that this “last gasp” apparently had lost sight of the “powerful magic” of the “Apollo program and the billion dollar economy.” Like most work, it was a cooperative effort, which in this case took shape largely in Pisa, in an international seminar with participants from all over Europe, culminating in a GLOW conference. That’s why my first and major book on the topic is subtitled “The Pisa Lectures”; perhaps the reason she does not cite it, given the story line.

The “last gasp” was followed by “Chomsky’s theory of minimalism,” which she dismisses as “risible” (on the basis of a quote from a hostile critic). It cannot be “risible,” because it does not exist. As has been explained ad nauseam, there is no “theory of minimalism.” Rather, there is a research program, which can be undertaken whatever theory

one favors, seeking to determine to what extent the nature of language and its acquisition follows from more general principles, including considerations of computational complexity, thus moving beyond the question of what is UG (the question of “explanatory adequacy,” in the terminology of the past 40 years) to the question of why language observes these principles and not others, and thereby embedding linguistics more fully within biology, including evolutionary biology. Perhaps Boden believes it is “risible” to investigate these questions, or that nothing has been achieved in doing so. Since she makes no effort to offer evidence, and seems to have no awareness of what appears in an extensive literature, I’ll again drop the matter.

Boden does manage one true statement in her reconstruction of the “Chomskyan” plague. She’s quite right that Chomsky “changed his mind more than once”; in fact, repeatedly. In other words, it is a living field, in which sane investigators are constantly changing their minds in the light of new empirical discoveries and theoretical insights, a regular occurrence.

Throughout, as in the cases mentioned, Boden’s claims are presented without any support. Her conception of an argument, repeatedly, is to quote someone who agrees with her judgments. QED. It doesn’t matter what the facts are.

For example, Boden would like to believe that “Chomskyans,” unlike structuralists, do not try to provide an empirical basis for their proposals about UG, outside of English (or “a small set of languages”). The total evidence for this conclusion is a statement by a Finnish phenomenologist who charges that “Chomskyans” are “obsessed with English,” and claims further, in Boden’s words, that “Chomsky himself would defiantly declare that ‘I have not hesitated to propose a general principle of linguistic structure on the basis of a single language’—an admission dubbed ‘preposterous’ by this critic.”

The defiant declaration given in quotes is presumably invented, but it doesn’t really matter, since the “preposterous” statement is a truism. Thus on the basis of a single language all linguists can, and do, conclude that UG permits *whin-situ*, emphatic consonants, transitive expletives, and on to much more abstract conclusions.

To be sure, it would be absurd to keep to English (or “a small set of languages”), as Boden claims is the practice of “Chomskyans.” That would indeed be a very severe criticism of a field of research that aims to discover UG within the general framework of the biolinguistic program that has been taking shape since the early 1950s, which regards linguistics as part of psychology, ultimately biology, and about which Boden seems unaware. However, even the briefest look at the literature refutes the allegation. My own earliest work on generative grammar in the late 1940s was on Hebrew ([4], 1979). The co-founder of our program at MIT and my sometime co-author has worked since the 1950s mostly on Russian (Morris Halle). The first large-scale generative grammar in our MIT program was on Hidatsa (G.H. Matthews). The first doctoral thesis (in the EE department, because there was no formal linguistics program then) was largely on Turkish (R.B. Lees). When the doctoral program was established, the range of languages under investigation quickly extended, and when Kenneth Hale joined the department a few years later (1967), it soon became a world center for study of Australian and American Indian languages. Richard Kayne’s 1975 doctoral dissertation founded the rich and lively modern study of generative grammar of Romance languages, inspiring similar developments in Germanic languages, all exploring entirely new ground, as was also true shortly after with Chinese, Japanese, and others too numerous and varied to mention. The crystallization of the “last gasp” P&P program in the early ‘80s led to an explosion of studies of languages of virtually every typological category, in depth previously unattained, leading to the discovery of a vast amount of new and surprising empirical evidence, and opening and sometimes answering questions that had never been even contemplated before.

But none of this matters. The phenomenologist has spoken. That settles the case.

Continuing, the designated dictator of the field is not satisfied with absurdity, but moves on to virtual lunacy, insisting that the only evidence for linguistics is his own judgments. In this case, Boden was apparently unable to discover someone to quote, so she simply proclaims that “Data-respecting critics soon complained—and still do—that by *native speakers* [Chomsky] means ... Chomsky,” no one else. “Such a source is hardly reliable,” Boden adds, far too charitably. Since facts are irrelevant to her history, it therefore doesn’t matter that one of my books [15] relies almost entirely on examples from Spanish, including subtle properties recently discovered in the extensive work by “Chomskyans” on Spanish generative grammar. Or that my writings often make use of examples from Italian, Icelandic, Chinese, and anything else that might be relevant. Nor does it matter that I’ve never hinted at the crazed belief Boden attributes to the Chomsky of her imagination, which is why she cites nothing. What is more, I have always stressed that while native speaker judgments from the widest possible variety of languages provide significant evidence for linguistic theory (just as judgments do for perceptual psychology), they by no means exhaust the relevant evidence, which might come from acquisition, aphasia, neuroscience, biochemistry, in fact any source; and I have

always sought to rely on such evidence when it appears, contrary to her repeated charges, issued as always without any evidence.

Boden goes beyond the charge of absurdity and lunacy to the ultimate damnation: vacuity. She triumphantly announces that it has been proven that transformational grammars “have the same power as a Turing machine,” and so can generate every language. Therefore the conceptions of “Chomskyans” are vacuous. Notice that even if the claim about Turing equivalence were true, the conclusion of vacuity would not follow, because of the associated evaluation procedure. But there is no reason to review the discussions of 50 years ago about that topic, because the claim is plainly false. Take the simplest imaginable language:  $\{a^n\}$ . Trivially, it cannot be generated by a transformational grammar—in the only sense of generation that has real linguistic significance, surely within the biolinguistic program. Superfluously, one may add that within the P&P framework that Boden briefly mentions, there are only a finite number of possible languages, so the claim could not be true under any interpretation.

Boden’s conclusion is based on a simple technical error, which may derive from her strangely skewed conception of cognitive science—in the case of language, focusing on formal language theory and computational studies, and only peripherally on language as a psychological/biological entity, and on the empirical study of language. The technical error is her failure to distinguish *strong* from *weak* generation. Within formal language theory, both can be defined, though virtually the entire subject deals with weak generation; strong generation, while definable, is too complex for much in the way of mathematical inquiry. The study of human language, in contrast, is concerned with strong generation. Perhaps one can construct a derivative notion of weak generation, but it appears to be of little interest (apart from being rather arbitrary, for reasons discussed in detail 50 years ago, reiterated today in what Boden regards as a “shocking” retraction). All of this was crystal clear half a century ago, so there should be no need for elaborate discussion.

In brief, a generative grammar strongly generates an infinite array of expressions, hierarchically structured, in fact considerably more richly structured than that. Each strongly generated expression  $E$  can be mapped to a linear string of symbols  $S$ , though the mapping is highly non-trivial. It incorporates all of morphology and phonology, and probably also the entire process of linearization, so it appears from much work of the past 30 years. The grammar then weakly generates  $S$ .

Since Boden shows little interest in human language, let us abstract away from all of this real world complexity and pretend that the generative grammar strongly generates hierarchically structured expressions  $E$ , and that each of them determines an unstructured weakly generated linear string  $S$ . For the study of human language, only  $E$  is of any significance: it is  $E$ , not  $S$ , that is mapped to the systems of thought and (having abstracted from morphology, phonology, and linearization) to the sensorimotor system. Furthermore, it is entirely unclear what the set  $\{S\}$  would be for some human language—again, for the reasons discussed 50 years ago. Thus, what is the status of so-called *deviant* sentences with their many dimensions of deviance, often perfectly understood and commonly used quite appropriately, and also the subject of some of the most important linguistic work of the past decades; the reasons for the striking difference between subadjacency effects and other kinds of deviance, for example.

The results to which Boden refers have to do with weak generation. Hence it is unclear what (if any) linguistic significance they have. That’s true of her alleged proofs of vacuity, generative capacity, learnability, in fact virtually everything to which she alludes, with one exception. She does mention my proof that she describes as “an insight that helped establish the theoretical basis of modern parsers, and of compiler design.” Correcting her misformulation, the result is that non-deterministic pushdown storage automata are *strongly equivalent* to normalized context-free (CF) grammars (a result later extended to all CF grammars); see [8], which she does not cite, for extensive discussion of this and other topics in formal language theory (“mathematical linguistics”). Note that the result had to do with *strong* generation, and therefore had some potential linguistic significance, as distinct from results about *weak* generation, which have interest for formal language theory, though it is unclear that they are of any significance for human language.

Boden’s confusion about these matters may result from her not having looked more than casually at any work by “Chomskyans” since my monograph *Syntactic Structures* (SS) [6] in 1957. And she apparently scarcely looked at that. SS does open with remarks about formal languages. The reason, as repeatedly explained in the years since, is that the monograph was based on notes for an undergraduate course at MIT, so it began with what was familiar to students: a narrow subclass of Markov sources, at the time widely assumed in psychology and engineering to be adequate for natural language (as Boden recognizes). The course notes then turned quickly to human language (strong generation),

also pointing out at once that weak generative capacity and the alleged grammatical-ungrammatical divide have no clear linguistic meaning (note 2).

At this point it might be useful to add an aside about publication history. Boden has great fun ridiculing the fact that publications were unavailable, part of her conspiracy so vast by the esoteric cult of “Chomskyans.” She’s right that publications were unavailable, for reasons that are worth a brief review, because they undermine a large part of her invented history of the origins of generative grammar.

The first extensive work on generative grammar, *LSLT* [5], was submitted to MIT press for publication, but turned down by reviewers on the reasonable grounds that there seemed to be no field to which it belonged. A revised 1956 version was published in large part in 1975, after the field had come to exist. Boden “confesses” that she does not have the mathematical competence to read *LSLT*, so relying on comments by hostile colleagues, she announces that the mathematical arguments were “maladroit,” “perverse,” and “plain wrong.” That is quite an achievement, since there are no mathematical arguments. This is linguistics, dealing with empirical properties of language. It is not formal language theory, where there are mathematical arguments (see [8]). There is some formalism in *LSLT*, but mathematics is about theorems, not formalism. And in *LSLT* any theorems are superficial.

Throughout, Boden fails to distinguish formal language theory from the study of human language, two quite different enterprises, with very little interaction, a failure that leads to repeated misunderstanding. Thus she claims that until the mid-1960s I declared that “I’m a mathematician,” and so my “‘pure’ mathematical theory isn’t under any threat from facts.” She makes no attempt to justify these remarkable charges. It is impossible even to imagine what her basis for them might be, or how she could have thought that my linguistic work from the 1940s to the mid-1960s was “pure mathematics,” divorced from facts about language.

Returning to publication history, for the reasons given by the MIT press reviewers, my undergraduate thesis [4], the first sketch of a generative grammar in modern terms, was also unpublished (a revised 1951 version appeared in 1979). The only article on my work I submitted to a professional linguistics journal in the 1950s came back virtually by return mail. *SS* was indeed published: in Holland, where publication in those days was very cheap and there were few constraints.

The reasons for the lack of publishing options were those given by the MIT press reviewers. Generative grammar seemed completely foreign to existing disciplines concerned with human language, contrary to Boden’s energetic efforts (as always, without evidence) to show that it was all borrowed from the prevailing structuralist approaches. Actually there was a tradition of something like generative grammar, later unearthed by “Chomskyans,” tracing from classical India to Leonard Bloomfield on Menomini in the late 1930s; see [2], another illustration of the “Chomskyan obsession” with English. But there is no hint of the tradition in the work of the structuralists she mentions, for a very good reason: it was completely foreign to their approaches to language, contrary to her unsupported assertions.

Boden writes that in *SS* Chomsky “claimed”—but, she stresses, did not *prove*—that phrase structure grammars were inadequate for natural language, on grounds (in her words) that they could apply, if at all, “‘only clumsily,’ by a complex, ad hoc, and unrevealing theory,” missing important generalizations. She repeatedly brings up the absence of *proofs* of this sort as if that were a serious defect, or a defect at all. That again reflects her lack of interest in human language—for that matter, in science generally, where such results are rarely *proven*. Rather, they are supported by evidence of exactly the kind she derides. That continues to be the case in the study of language, with marginal exceptions, as throughout cognitive science (and the sciences more generally). One cannot possibly *prove* the adequacy, or inadequacy, of theories (apart from inadequacy of the most trivial cases), in an open and developing field, with new material and ideas constantly being produced and explored. It is familiar to every serious practitioner that a great deal remains unknown, and poorly understood, about even the best-studied human languages. To ask for proofs is to reveal considerable misunderstanding of the nature of empirical inquiry.

Boden then turns to the “Battle with Behaviorism.” The battle is joined in her section entitled “That review!”—my 1959 review-article on B.F. Skinner’s *Verbal Behavior* and (she omits this part) on alternative approaches in psychology and biology that seemed more promising to me [7].

Boden’s account of my “relentless” battle with behaviorism opens with a section entitled “Political agenda,” in which she asserts that I “saw [behaviorism] in political terms” and was driven by “political passion.” That is another serious charge, based as usual on zero evidence. The 1959 article has not even the remotest hint of any political concern. Boden’s evidence about a “political agenda” comes from a more general article on psychology and ideology many years later [11] that dealt in part with Skinner’s *Beyond Freedom and Dignity*. Since Skinner’s book had a very explicit “political agenda,” my review, like every other one, saw it “in political terms.” The charge that my linguistic

work reflects a “political agenda” is repeated throughout, on the basis of her usual source: quotes from a hostile critic, backed by no evidence. Readers may judge for themselves the intent of these fabrications.

My “relentless” battle consisted of these two articles and a few scattered remarks here and there, including an explanation of why I had little interest in behaviorism.

Boden then turns to “That review!,” which, she concedes, was correct in observing that Skinner’s far-reaching conclusions collapsed if his terms were given their technical meaning, and relied on “mentalistic intuitions” if taken metaphorically—more accurately, gave a very poor translation of much richer mentalistic concepts. But to compensate for the concession, she quickly explains that I was “an enthusiastic latecomer” to the “growing revolt against behaviorism,” citing particularly the important work of Karl Lashley, which, she says, was missed by cognitive scientists until the 1960s when “his lead was acknowledged by Miller and Chomsky” (apparently referring to [23], in which Lashley is not mentioned).

Boden sidesteps the fact that the most detailed discussion of how I was “an enthusiastic latecomer” was in “That review!,” which, furthermore, did not merely “acknowledge” Lashley’s many contributions, including his very significant “serial order” paper, but argued that his unfortunately neglected work offered a much more fruitful approach than Skinner’s for cognitive science, including the study of language. That’s quite impossible to miss in “That review!” That is how the final section opens. In the same connection, I also cited the “revolt against behaviourism” in contributions of Tinbergen, Thorpe, Lorenz, Harlow, Birch and Bitterman, Jaynes, Schiller and others, suggesting that this work offers a much more promising course than Skinner’s approach, then widely accepted as authoritative if not conclusive, not only in the “behavioral sciences” but also in philosophy, particularly because of its adoption by W.V. Quine in classes and lectures through the 1950s, culminating in his influential *Word and Object* [25].

Boden does include a section entitled “Some surprises from ethology,” reviewing work of the past 30 years which, she says, undermine the beliefs of psychologists. She neglects to mention that the same is true of work of ethologists in much earlier years, as discussed in “That review!”

Boden writes that the truly shocking part of “That review!” was the claim that “the child’s language acquisition can’t be fully explained by experience.” That hardly rises to the level of a “claim,” since it is the merest truism, holding for every other aspect of cognitive (and other) growth as well. Surely Boden accepts it.

Boden reports that this shocking truism elicited debates about “nativism” through the 1960s (and beyond). She fails to mention, however, that the debates were one-sided. There was extensive critique of “nativism” and its “innateness hypothesis,” but no defense of it, because there is no such general hypothesis, beyond the truism that the human language faculty has a genetic component, the topic of UG. In particular, I did not participate in these debates, apart from responding to invalid charges and observing that the questions are empirical: how does acquisition take place, relying on experience, the genetic endowment (UG), and the general biological (and broader) principles that enter into all growth and development?

Outside of these pointless one-sided debates there were real debates about the exact nature of UG—hence about the substantive content of “nativism,” a misleading term avoided by participants. These of course continue, and will into the indefinite future—as in every other area of cognitive science, and science more generally. But these do not fall within cognitive science in Boden’s construal of the discipline, so she virtually ignores them.

Boden informs us that the vigorous debates about “nativism” and the (non-existent) “innateness hypothesis” were effectively settled when one of the leading participants, W.V. Quine, “in characteristically gentlemanly fashion, mildly declared” that behaviorism cheerfully accepts whatever “innate structure is needed . . . to account for language learning” [26]. That is correct, but she fails to add that in this (quite uncharacteristic) comment on a paper of mine, in a symposium, Quine completely retracted all of his relevant positions, including his influential doctrine of indeterminacy of translation and any relation of his work to behaviorism (which now becomes merely the thesis that conjectures must “eventually be made sense of in terms of external observations”—that is, weak verificationism). Nor does she mention that in this brief paper, Quine also retracted, in its entirety, his sharp critique of linguistics, my work in particular. While during the years of the debate he had argued forcefully that generative grammar is irremediably flawed on fundamental methodological grounds, in the 1969 paper he states that “generative grammar is what mainly distinguishes language from subhuman communication systems” (having now withdrawn the basis for his methodological critique).

So indeed the debate ended, but not exactly in the way Boden describes the matter in her history. Since all of this has been discussed in detail in print long ago ([13], which Boden cites), there is no need to review the matter here.

Boden then turns to a section with the exciting title “Transformations trounced.” Here at least we begin to approach a matter of substance for the study of language: namely, what is the real nature of UG? Before proceeding with Boden’s account, however, it is necessary to fill in some background that she missed, having virtually ignored the work of “Chomskyans” since the 1950s, while misunderstanding the little to which she alludes, as already reviewed.

The first generative grammar in the modern sense, my undergraduate thesis ([4], 1979), consisted of a phrase structure grammar enriched by devices to express long-distance dependencies. In the 1950s, I came to the conclusion that far better empirical results could be achieved by eliminating the additional devices in favor of transformational rules that entered into the derivation of all expressions. That is the picture developed in *LSLT* and *SS*. By the 1960s, it became clear that context-free phrase structure grammar is too rich and complex, and involves far too many arbitrary stipulations. That led to X-bar theory, which by 1970 eliminated rewriting rules altogether. To adopt Boden’s evocative terminology, by then phrase structure grammar had been “trounced.”

Transformations however survived, for fundamental reasons that became clear later on. Subsequent work, which I will not review since Boden scarcely mentions it, greatly simplified transformational rules by abstracting general principles, and within the minimalist program, showed that the last remnants of phrase structure rules could be eliminated without loss, in fact with gains (“bare phrase structure”), if the computational principles of language keep to the simplest possible operation that generates hierarchically structured expressions (called “Merge”). It was later recognized that keeping to Merge, transformational rules “come free.” They are automatically available; it would require a stipulation to bar them. More precisely, what “comes free” is the so-called copy theory of movement, well supported empirically, particularly in important studies of the rich and complex semantic phenomena of “reconstruction.” For one of many reviews, see [18].

As matters have stood for some years now, empirical evidence would be required both to support a stipulation barring transformational rules, and in addition, to justify any devices beyond Merge (including phrase structure grammar, or any variant or additions to it) that are introduced to account for “displacement” phenomena: the ubiquitous fact that expressions are pronounced in one position but interpreted somewhere else as well, in what turn out to be intricate ways. Boden ignores these developments, keeping to the framework of the 1950s, which accepted phrase structure grammars (soon abandoned as far too complex and arbitrary) and focused on transformational rules. Accordingly, even her unsupported beliefs about burden of proof are exactly backwards—and she presents no hint of evidence that it has been met.

How then were “transformations trounced” in Boden’s version of history? By the development of non-transformational enriched phrase structure grammars in the 1970s. But by that criterion, transformations had already been “trounced” in my 1949 undergraduate thesis. Of course it was never “proven” that such enriched phrase structure grammars were inadequate, because there are no such proofs in an open and developing empirical science apart from the most elementary examples, as already noted.

Boden claims further that the originator of one of these 1970s theories (GPSG), her colleague Gerald Gazdar, concluded that GPSG “could describe the whole of English” and “seemed to fit about twenty diverse tongues.” Boden must have misunderstood. No one who has the slightest familiarity with human languages could ever make such a claim about any theory of language, for familiar reasons already mentioned. New empirical discoveries are constantly being made about even the most extensively studied languages and new puzzles unearthed, and many long-standing problems are still far from resolution. The most casual look at the literature suffices to demonstrate that—and it should be evident even without inquiry.

Boden is also greatly impressed by the reducibility of GPSG to CF grammars, a matter with “deep implications” because it demonstrates the “computational tractability” of GPSG, a fact so important it is elevated to a section heading. It has been known for 20 years that these systems are computationally *intractable*, and even run into undecidability problems [1,27,28]—not a serious criticism in an empirical discipline that is open-ended, with new materials constantly becoming available and major problems unresolved, in which theories are put forth tentatively as “best guesses” as to how to advance understanding.

As noted, these sections of Boden’s history at least verge on questions of real significance: what is the nature of UG? These questions have to be addressed by the criteria she derides, those of normal scientific inquiry: explanatory success, discovery of general operative principles, etc. Unfortunately, she does not engage any of the questions, so there is no point proceeding.

Let’s turn then to the really exciting denouement, the section headed “Linguistics eclipsed.” Since linguistics is the study of human language, it is a remarkable feat to have “eclipsed” it. How was that achieved?

The crucial step in eclipsing linguistics was my 1965 distinction between *competence* and *performance*, which “put a theoretical firewall between the psychology of language and linguistics,” and in effect “instructed [linguists] to take no heed of psychology,” while psychologists were “tempted to regard abstract linguistics as largely irrelevant to their concerns.”

Once again, Boden misunderstands a simple notion. The term “competence” is a technical term corresponding to what is informally called in English “knowledge of language.” The technical term was chosen, as explained, to dissociate the concept from irrelevant connotations in the philosophical literature bringing in beliefs and propositional attitudes. “Performance” is behavior; again, as explained, the term was chosen to avoid misunderstanding induced by behaviorist literature. The distinction, then, is between what we know and what we do. It is a conceptual distinction, accepted by everyone, Boden included, and either tacitly or explicitly adopted in all work in psychology of language. Take, say, the rich and important studies of acquisition of language, which are intimately interrelated with research on generative grammar, as Boden could have learned by the most casual investigation of the literature. This work of course breaks her “firewall” and violates the “instructions” allegedly issued by “high priest,” who, irrelevantly as always, has always radically rejected Boden’s remarkable construal. The study of acquisition of language is the study of acquisition of *competence*, of what the child comes to know. And plainly that is distinct from particular acts, performance. Citation should be superfluous, but see, e.g., [19–21,31]. The same is true elsewhere in psychology of language.

In her rare excursions into these areas Boden perversely insists on being wrong, if it helps the story line advance. Thus “most Chomskyans didn’t care” about garden path sentences like “the horse raced past the barn fell,” she declares. That will be news to “Chomskyans,” at least if Thomas Bever and many others are among the sinners (which, of course, they would hotly deny, rightly, as would everyone). The example she cites is Bever’s, brought up in a 1970 paper that elicited a great deal of work and discussion on these topics [3] (for review, see [30, Chapter 7]).

The reason “most Chomskyans didn’t care” in Boden’s imaginary world is that “They’d retreated to the mathematics.” If this is not pure invention, it can only be another illustration of Boden’s apparent inability to distinguish formal language theory from linguistics. Boden also suggests that the “Chomskyan orthodoxy” that acquisition and processing involve generative rules was undermined by connectionist past-tense learners. She does not, however, cite Charles Yang’s demonstration that even in this marginal area of language acquisition, more careful analysis of the data provides strong evidence for access to phonological rules [31].

Another Boden discovery is that “Chomsky claimed that there could be no evolutionary explanation of language.” She cites an actual source in this case, so we can determine at once that Chomsky states the exact opposite. The quote she gives states that the evolution of language may involve “‘emergence’—the appearance of a qualitatively different phenomenon at a specific stage of complexity of organization.” If so, it would be an interesting, but by no means novel, case of evolution. A similar view is widely held by evolutionary biologists and paleoanthropologists, for example, Ian Tattersall, who suggests more generally that human intelligence is an “*emergent* quality, the result of a chance combination of factors, rather than a product of Nature’s patient and gradual engineering over the eons” [29]. Still more generally, neuroscientist Vernon Mountcastle, introducing an American Academy of Arts and Sciences collection of essays on the state of the art at the conclusion of “the decade of the brain” that ended the last century, formulates the leading principle of these contributions as the thesis that “Things mental, indeed minds, are *emergent* properties of brains [my emphasis], ... produced by principles ... we do not yet understand”—and that might derive from laws of nature [24]. Not an original thesis, but one that recapitulates, virtually in the same words, the conclusions of chemist/philosopher Joseph Priestley two centuries earlier, expressing a common understanding among scientists and philosophers after Newton’s demolition of the “mechanical philosophy,” the conception that the world is a machine [16,17].

Boden adds that my “pessimistic remarks” just quoted “implied that ‘emergence’ is inexplicable,” and adds that “Similar anti-evolutionary scepticism has often been associated with the complex structure of the eye,” refuted by Walter Gehring’s work—which, as it happens, is quite consistent with my pro-evolutionary non-scepticism.

Boden proceeds with other “Chomskyan” quotes about evolution that she finds puzzling or worse, raising further questions about her beliefs about evolution, since none are puzzling at all in the context of evolutionary biology. Her interpretation of these remarks is particularly surprising since they correspond so closely to the well-known ideas of one of her heroes, Alan Turing (which she cites), now gaining new prominence in the “evo-devo revolution” in biology.



When Boden turns to history, she sometimes makes correct statements, but constantly twists them to fit the picture she is attempting to construct. Consider her main criticism: my citation of Humboldt and Descartes on the “creative aspect of language use.” The reference is invalid, she argues, because “Chomsky’s concept of creativity fails to capture all the cases covered by the term, including those which Humboldt seems to have in mind.” Again mistaken, and hopelessly muddled. In fact, “Chomsky’s concept of creativity” of language use—repeat *use*—includes all the cases, but the substantive proposals I discussed do not capture *any* of them, for obvious reasons. Furthermore, all of this has been clear, explicit, and unambiguous in what I have written about these topics since the 1960s. These publications stressed that the figures I cited, from Descartes and his predecessors through Humboldt, were concerned with the creative *use* of language, while the work I was presenting dealt with the *mechanisms* that enter into language use, a crucial difference. The publications also made clear, repeatedly, the distinction between the normal creative use of language, which was the primary concern of those I discussed, and the higher capacities involved in true creativity (see [9,10], and often since, also tracing these distinctions back to predecessors of the Cartesians).

In fact, I even devoted a 1982 article in a journal Boden knows well to refuting the claims that she repeats [14]. The article reviews my writings on the topic in the 1960s, which invariably make clear and explicit—to quote—the distinction between “the normal creative use of language,” about which “we cannot now say anything particularly informative,” and “the mechanisms that make possible this creative use of language,” which “we are slowly coming to understand” (1969, reprinted in [12]).

Even where Boden’s historical points, intended as criticism, are correct, they refute claims that were not made and miss the point that was made: for example, with regard to the Port Royal distinction between meaning and reference—“in pretty much their contemporary sense,” in my words—“pretty much” being different from “exactly,” undermining her criticism. The point of the discussion, which she omits, was that the Port Royal grammarian-logicians made use of this distinction to explain a property of relative clauses in the vernacular that was a long-standing concern. The remainder of her account of my discussions of historical backgrounds proceeds in the same vein. To see how consistently her main charges miss their intended target it suffices to reach pages 2–3 of [9].

There seems to be no purpose in continuing to unravel each individual case. Not only do her efforts fail throughout for similar reasons, but she also scrupulously avoids something relevant that she surely knows, as a self-identified specialist in this earlier period: that earlier work, including scholarship, either ignored the historical record I discussed or seriously misrepresented it. That would, of course, be no excuse for error, but since she fails to show anything of the sort, the question is moot.

I won’t render any judgments on the rest of the two volumes, for the reasons mentioned at the outset.

## References

- [1] G.E. Barton, R. Berwick, E. Ristad, *Computational Complexity and Natural Language*, MIT, 1987.
- [2] T.G. Bever, Theoretical implications of Bloomfield’s “Menomini Morphophonemics”, *Quarterly Progress Report*, R.L.E., MIT, 1963.
- [3] T.G. Bever, The cognitive basis for linguistic structures, in: R. Hayes (Ed.), *Cognition and Language Development*, Wiley, 1970.
- [4] N. Chomsky, *Morphophonemics of modern Hebrew*, Ms, undergraduate thesis, U. of Pennsylvania, 1949. A revised December 1951 version was published in: J. Hankamer (Ed.), *Outstanding Dissertations in Linguistics*, Garland, 1979.
- [5] N. Chomsky, *Logical structure of linguistic theory*, ms, 1955. Partially edited 1956 version, available in microfilm and Pdf form, was published in large part in 1975, with an explanatory introduction, Plenum, Chicago.
- [6] N. Chomsky, *Syntactic Structures*, Mouton, 1957.
- [7] N. Chomsky, Review of B.F. Skinner, *Verbal behavior*. *Language* 35 (1) (1959).
- [8] N. Chomsky, Formal properties of grammars, in: [22], 1963.
- [9] N. Chomsky, *Cartesian Linguistics*, Harper & Row, 1966.
- [10] N. Chomsky, *Language and Mind*, Harcourt Brace Jovanovich, 1968.
- [11] N. Chomsky, Psychology and ideology, *Cognition* 1 (1) (1972).
- [12] N. Chomsky, *Language and Mind*, enlarged edition, 1972.
- [13] N. Chomsky, *Reflections on Language*, Pantheon, 1975.
- [14] N. Chomsky, A note on the creative aspect of language use, *Philosophical Review* XLI (3) (July 1982).
- [15] N. Chomsky, *Language and Problems of Knowledge*, MIT, 1987.
- [16] N. Chomsky, *New Horizons in the Study of Language and Mind*, Cambridge, 2000.
- [17] N. Chomsky, *On Nature and Language*, Cambridge, 2002.
- [18] N. Chomsky, Three factors in language design, *Linguistic Inquiry* 36 (January 2005).
- [19] S. Crain, R. Thornton, *Investigations in Universal Grammar*, MIT, 1998.
- [20] M.-A. Friedemann, L. Rizzi (Eds.), *The Acquisition of Syntax*, Longman Linguistic Library, 2000.
- [21] M.T. Guasti, *Language Acquisition: The Growth of Grammar*, MIT, 2002.

- [22] D. Luce, R. Bush, E. Galanter (Eds.), *Handbook of Mathematical Psychology*, II, Wiley, 1963.
- [23] G. Miller, N. Chomsky, Finitary models of language users, in: [22], 1963.
- [24] V. Mountcastle, Introduction to “The Brain”, Daedalus, 1998.
- [25] W.V. Quine, *Word and Object*, MIT, 1960.
- [26] W.V. Quine, Linguistics and philosophy, in: S. Hook (Ed.), *Language and Philosophy*, NYU, 1969.
- [27] E. Ristad, Defining natural language grammars in GPSG, AI memo 895, MIT Artificial Intelligence Laboratory, April 1986.
- [28] E. Ristad, GPSG-recognition is NP-hard, AI Memo 837, MIT Artificial Intelligence Laboratory, August 1986.
- [29] I. Tattersall, Patterns of innovation in human evolution, *Evolution und Menschenwerdung*, Nova Acta Leopoldinia 345 (93) (2005).
- [30] D.J. Townsend, T.G. Bever, *Sentence Comprehension*, MIT, 2001.
- [31] C. Yang, *Knowledge and Learning in Natural Language*, Oxford, 2002.